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SECTION OF GEOLOGY AND MINERALOGY

FEBRUARY 2, 1942

PROFESSOR HORACE ELMER WOOD, 2ND., University of Newark and The American Museum of Natural History: *Problems of our Continental Tertiary.*[1] (This lecture was illustrated by Kodaslides).

The late W. D. Matthew (1915) contributed an essay on "The Tertiary sedimentary record and its problems" to the Dana Memorial "Problems of American Geology." It is interesting to compute the extent of progress since the start of the first World War, as we are now, a generation later, fairly launched on an even greater war. This essay remains the most readable general account of the American continental Tertiary and still contains much that the specialist can profitably read. Matthew was much exercised to convince his reader that the continental deposits were

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flood plain and aeolian, in contrast with the lacustrine dogma of last century. This battle is won at last and need not detain us longer. Perhaps we have not yet fully caught up with Matthew's analysis of the magnitude of the direct and indirect contribution made by volcanic ash to the western sediments, but we are moving in that direction. His centrifugal distribution of life from the Holarctic land mass out the three divergent prongs of South America, Africa, and Australasia has become a basic part of our thinking, now largely divorced from such theoretical elaborations as homotaxis and the necessarily peripheral location of primitive survivors.

Matthew protested against correlating faunal lists mechanically, by the degree of similarity or dissimilarity, without regard to the reliability of the identifications or the degree of completeness of the material on which the names were based. His warning deserves an audience today, although professional practice has improved considerably, partly due to Matthew's protests. Intercontinental correlation has made only modest advances since this time. We know about the same things but with a little more confidence due to additional confirmatory evidence. The Mongolian discoveries have provided new lines of evidence which have permitted more satisfactory correlation either with the European record, or with our western deposits, or with both.

This generation has seen the rise, and, perhaps, the decline of the Taylor-Wegener hypothesis of continental drift. In contrast with the unconsciously misleading use made by Wegener of paleontological arguments drawn from standard texts (such as Osborn's "Age of Mammals," 1910), no mammalian paleontologist, so far as I know, has been converted to "Continental Drift." For the specialist on the Cenozoic, continental drift would raise far more, and more serious, new difficulties than the old ones which it would apparently solve. This, of course, has no bearing on whether the continents might have drifted apart before Cenozoic time, or, for that matter, whether they may have alternately secured a divorce and then nestled affectionately together again. But the mammalian record contradicts any such rapprochement

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during the last sixty million years or so. However, there is no question that this hypothesis has stimulated observation as well as discussion and therefore illustrates how even a probably fallacious generalization may proove stimulating.

Some progress has been made in attempts to intercorrelate the marine and continental sediments, and additional solid progress may be anticipated. Early slipshod work in Florida and the Texas Coastal Plain has been or is being straightened out, important evidence has been obtained from these areas and from California, and more may be expected. Nevertheless, it is not all peaches and cream, and the new titanothere from Mississippi (Sullivan and Gazin, in press) appears to be of very late Eocene aspect according to the continental standard, but associated with middle Eocene marine fossils. This recalls a similar embarrassing quandary regarding the not too primitive titanothere, *Brachydiastematherium transsylvanicum*, and the true rhinoceros, *Prohyracodon orientalis*, from the variegated clays of Andrashaza (Wood, 1929). Andrashaza Hill is near the city variously known as Klausenberg, Kolozsvár, and Cluj, which, once in Hungary, then in Rumania, and recently returned to Hungary, is even more uncertain of its ultimate geographic location than is the geologic level of its mammalian fossils. The horizon of these two mammals (which appear late Eocene or even early Oligocene in morphologic character) is underlain by a late lower Eocene marine fauna and overlain by another marine fauna of middle Eocene affinities. This anomalous situation at Andrashaza has been known since the end of the last century, but has usually been ignored or explained away rather than faced squarely. It is, of course, in the middle and upper Eocene that correlation between the marine and continental sequences has been—and remains—most dubious and is largely based on indirect lines of evidence. Nevertheless, these discrepancies are difficult to explain.

It is interesting to compare Matthew's correlation chart (1915, p. 412) with a recent summary (Wood *et al.*, 1941, Pl. 1). There are new occurrences and greater refinement in detail, throughout, but it is striking how little the Puerco and Torrejon,

the standard Eocene succession, and the Oligocene to lower Miocene sequence, have changed in a generation. The Paleocene epoch has at last been recognized by the U. S. Geological Survey and other conservatives, and its boundaries have been satisfactorily delimited. The upper Paleocene has become progressively better understood. The Loup Fork, which had only just been recognized as a scrap basket where an apparently precise name operated to prevent precise observation or communication (Osborn and Matthew, 1909) has now been fully resolved into its component parts, and the Pliocene, until recently a hopeless hash (or, if you prefer a pleasanter foreign word, "mélange"), now presents successive faunas showing stages of autochthonous evolutionary advance as well as immigrations from other continents. Thus the Blanco and Hagerman faunas are now recognized as late Pliocene and a new middle Pliocene has been segregated out of the amorphous "Loup Fork."

The dispute over period and epoch boundaries is perennial and remains about where Matthew left it. Most of us would agree that time is continuous and that geological time boundaries are as arbitrary as the distinction between 11:59 P. M. of one day and 12:01 A. M. of the next. Nevertheless, the question seems to exercise an undying fascination and frequently generates heat without light. Legalistically speaking, epoch boundaries are based on the European standard, but there has been almost as much difficulty in securing agreement among European geologists about geologic boundaries as there seems to be, currently, about national boundaries in the same continent. Perhaps, if the larger and more immediate problem should be satisfactorily settled, it might then be possible to secure agreement on whether the Aquitanian is "really" Oligocene or Miocene, and the Pontian is "really" Miocene or Pliocene, but I am not too optimistic. Further complication is furnished in this country by some parochial geologists, whether state officials or self appointed local patriots, who feel that epoch boundaries must "naturally" coincide with the levels that would most conveniently fit the deposits of their particular state.

A school of geologists has claimed that geologic time divisions are "naturally" delimited by diastrophism. This concept, which assumes as self-evident that diastrophic activity in diverse parts of the world jerked along by synchronized fits and starts, represents a surprising reversion toward Cuvierian catastrophism.

(As the rest of this address was illustrated by Kodachrome slides, much material is omitted that would be unintelligible without illustration.)

Correlation and mapping has always been, and doubtless always will be, based to a considerable extent on the general aspect of the rocks. This is particularly true of continental sediments, in which local fossil pockets are often separated by miles of completely barren sediments, and in which the color, the pattern of weathering and erosion, and other lithological peculiarities may be very striking. It is a commonplace that we may be completely misled by such criteria, but we can not avoid trusting to them in the absence of fossils, and, even when fossils occur, the evidence from the sediments themselves should not be ignored. Sometimes, the boundaries apparently indicated by color and other lithological peculiarities are not substantiated by the faunal boundaries established later. This is the case for the contact between the Uintan and Oligocene along Beaver Divide on the south side of the Wind River Basin, and between the upper Eocene Sage Creek formation and the middle Oligocene Cook Ranch of southwestern Montana. Evidently, something happened during Uintan time which was reflected in a sharply changed lithology in these two areas and in the Uinta formation of Utah. A little later, between Eocene and Oligocene, comes the sharpest break in the whole Tertiary continental sequence, with the bulk of deposition shifting from the intermontane basins to the High Plains. Again, deposits demonstrated to be contemporaneous by their fossils may differ completely in lithology, especially if they are a considerable distance apart. There is not the slightest resemblance between the Torrejon formation of the San Juan Basin of New Mexico and the contemporaneous Rock Bench of Park County in northwestern Wyoming; nor between the Arikaree of Nebraska and the

channel sandstone of comparable age containing *Diceratherium*, at the mouth of the Madison River in Montana, north of Yellowstone Park; nor between the Madison Valley formation of the same area and the contemporaneous Barstow of the Mojave Desert of California. Or, rocks of similar aspect may be of exceedingly different ages, as illustrated by the striking color similarities of phases of the Chugwater and Lysite of Wyoming; although this may not be pure coincidence, as it has often been suspected that the Lysite colors are directly derived from the Chugwater. In all these and other problems, it would seem that the paleontologist might better call on the sedimentary petrologist for collaboration, rather than try to master that far removed and highly complicated field for himself.

The perennial Wasatch problem has now nearly ceased to be one of fact, and has become one of popular education; that is, of educating the non-specialist geologist. The text books picture the "Wasatch" with a distinctive fauna, which fauna (the Gray Bull) has never been found in the type Wasatch group and is quite probably represented there only by the hiatus below the Knight formation. On the other hand, the books tell us that the Wind River formation overlies the Wasatch and contains two later faunas—those of the Lysite and Lost Cabin members. However, both these faunas and no others are now known in the Knight and, if other workers agree in treating Love's Indian Meadows formation (1939) as the basal member of the Wind River formation, the Wind River not merely correlates with the entire Knight, but also includes a lower member (equivalent to the Gray Bull) which contains what most people have called the "Wasatch" fauna, unknown, however, in the type Wasatch. This leaves out of account the age of the probably late Paleocene Almy and the dubious Fowkes, which are, by definition, part of the type Wasatch group, but hardly belong to the standard Wasatch concept, which is better expressed in the modified form of "Wasatchian," *i.e.*, lower Eocene of the Rocky Mountains.

In recent years, the Green River formation has begun to present a conceivable picture of possible real conditions (especially

due to Bradley's work), as opposed to a cut and dried text book lake deposit. The complete separation of the northern and southern lakes has been emphasized by increasing evidence (particularly from the Carnegie Museum expeditions) that the northern lake was completely filled or drained before the southern lake was well started. Interfingering between the Green River and the contemporaneous continental deposits was first shown by Sears and Bradley (1924), and additional cases have been discovered by Jepsen, east of Labarge, Wyoming, and by me, between Boulder and Big Piney, farther north in the same region. The conception of the complicated relations between lacustrine and continental sediments formulated by Nace (1939) will probably win general acceptance in time.

Slim Buttes, in northwestern South Dakota, presents an interesting structural problem. It seems startling to find Great Plains Oligocene apparently folded or faulted into steep dips (Todd, 1895). Winchester (1913) interpreted these structures as large scale crossbedding, distinguishing this old structure from recent slumping, which he recognized around the edge of Slim Buttes. Toepelman (1923) explained the earlier structures by analogy with this slumping, as caused by "pre-Miocene" slumping, and this interpretation seems to be accepted in later literature. This hypothesis requires deep, steep walled canyons cut and maintained in the unconsolidated White River sediments, followed by slumping, in which these (by his hypothesis) weak sediments behaved as sufficiently cohesive structural units to simulate text book fault blocks. How a landslide actually proceeds is illustrated by the Gros Ventre landslide; and slow slumping, by the recent slumping around the edge of Slim Buttes. Apparent support for the explanation of essentially contemporaneous slumping was also furnished by the supposed age of the overlying cliff-forming sandstone, which was tentatively referred, without fossil evidence, to the Arikaree; since this assignment allowed time for erosion and essentially contemporaneous slumping, but little time for diastrophism and peneplanation. A fossil rodent in the Carter County Geological Society museum, collected from the same formation

where it caps Chalk Buttes near Ekalaka, Montana, is of late Miocene aspect. This new date for the massive sandstone leaves ample time for diastrophism, followed by peneplanation, previous to the deposition of the caprock.

For more than a generation, *Daemonhelix* has been interpreted, alternately, as a plant growth and as a fossilized rodent burrow. Just as it seemed that the burrow interpretation had finally won by common consent, Lugn reinterpreted this spiral structure as a liane (1941). Certainly, many of the vertical spirals resemble climbers, but one is startled by the change in ecological conditions which Lugn thus postulates for the Harrison, from the currently accepted temperate plains to a marshy tropical or subtropical forest. The corroborative evidence for this new interpretation seems inadequate.

Correlation of the Tertiary sediments may help the structural geologist to solve the problem of the Rocky Mountain basins and their mountain rims. Thus, Lysite Mountain, which lies virtually on top of the southern hook of the Bighorn Mountains, is composed of sediments identical with the Green Cove and Uintan of the Wind River Basin. It is difficult to believe either that the Wind River and Bighorn basins were once filled level with the top of Lysite Mountain, or that there has been post-Eocene differential movement, between the Mountains and the Wind River Basin, that could be measured in thousands of feet (as opposed to movements totalling some hundreds of feet which have evidently taken place). Again, sediments which, so far, have proved unfossiliferous, but are vaguely of White River aspect, lie on top of the Bighorn Mountains, west of Buffalo, at altitudes up to 9,000 feet. One would be rash to fill the Powder River Basin (in his mind's eye) deeply enough with sediment to accord with this and the scattered White River outliers in the Black Hills (*e.g.*, at Lead). However, the almost buried Granite Mountains of central Wyoming, and the rugged topography of the Absarokas partly buried beneath the Aycross formation, do show how mountain ranges may be virtually buried in their own débris, with or without the assistance of volcanic ash. The extent to which the Bighorn

Mountains were actually buried also virtually decides whether we should interpret the anomalous course of the Bighorn River, through the Wind River Canyon, as antecedent (thereby neatly begging the question), as superimposed during a time of filled basins, or as a result of piracy along the Boysen fault. With either of the latter explanations, regional tilt to the northeast would also be a major contributing factor.

It is regrettable but true that no progress can be reported on the exasperating problem of the relatively sudden extinction of the large reptiles at the end of the Cretaceous, and their replacement, rather shortly thereafter (so far as the stratigraphic record indicates—particularly at Polecat Bench, Park County, Wyoming), by an apparently new mammalian fauna with no known ancestry in the Cretaceous.

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SECTION OF BIOLOGY

FEBRUARY 9, 1942

PROFESSOR BRENTON R. LUTZ, Department of Biology, Boston University, Boston: *The Neuro-Motor Mechanism of Small Blood Vessels.* (This lecture was illustrated by motion pictures and lantern slides).

Are the capillaries merely inert thin-walled tubes dependent on the arterioles for their blood supply, or are they independently active, with a capacity to respond individually through nervous and hormonal control? On the answer to this question may depend the correct interpretation of many physiological and pathological conditions, such as muscular activity, increased glandular secretion, the stopping of hemorrhage, inflammation, traumatic shock and Raynaud's disease. Krogh (1919) pointed out that a resting skeletal muscle may have less than 100 open capillaries per square millimeter, while the increased oxygen demand of an active muscle is met by the opening of more than 2500 capillaries in the same cross-sectional area.

Several mechanisms for controlling capillary blood flow have been described. In Krogh's laboratory, Vimtrup (1922) reported the contraction of Rouget cells on the capillaries, which caused folding of the endothelium. In a review of the literature on the regulation of capillary blood flow, Krogh (1929) presented the evidence for the concept of independent contractility of the capillaries. Recent investigators using improved techniques have come to no general agreement concerning this concept or the mechanism involved. Kahn and Pollak (1931), working on the excised nictitating membrane of the frog, noted the protrusion of endothelial nuclei into the lumen, and the longitudinal folding of the endothelium in the region of pericapillary cells. Using the frog and rat, Field (1935) reported capillary closure in two ways: first, the swelling of endothelial nuclei which blocked the lumen of

the capillary; and, second, the contraction of pericapillary cells (Rouget cells). Beecher (1936) confirmed Field in studies on the blood vessels within transparent chambers inserted in the rabbit's ear. Using a similar preparation, Sanders, Ebert and Florey (1940) reported the swelling of endothelial nuclei following electrical stimulation of the peripheral end of the cut cervical sympathetic nerve. Clark and Clark (1940) observed the contraction of endothelial cells in the tadpole's tail, but not in the rabbit's ear. They have not seen the contraction of pericapillary cells, but concede that it may take place in certain tissues, such as the nictitating and hyaloid membranes of the frog. The new technique of micro-manipulation, as applied to capillaries by Zweifach (1934), made it possible to study this problem by direct methods. Zweifach (1939, 1940) maintained that capillary blood flow was regulated chiefly by the contraction of small arterioles, and that changes in capillary caliber were merely passive.

In a preliminary report, Fulton and Lutz (1940) stated that capillary origins in the retrolingual membrane are provided with modified smooth muscle cells, which may regulate capillary blood flow in a sphincter-like manner independently of the supplying vessel. The present paper presents further evidence for this view, along with the results of micro-stimulation, recorded by the motion picture camera, which support the concepts of dual innervation of the small blood vessels and of discrete vascular smooth muscle motor-units. Since 1937, Dr. George P. Fulton in our laboratory has confirmed and extended my original work.

Method

The blood vessels in the retrolingual membrane of the frog, *Rana pipiens*, with brain and medulla destroyed, were prepared for use after the method of Pratt and Reid (1930). In this membrane, blood vessels, nerves and skeletal muscle fibers are arranged in a thin sheet between two protective layers of epithelium. From the dorsal surface of the tongue, sufficient papillary epithelium and skeletal muscles were removed to expose the membrane beneath. The frog was placed in a Petri dish, containing a glass

block cemented to the bottom. The tongue was everted so that the upper surface of the retrolingual membrane rested on the block. Sufficient Ringer's solution was added to cover the membrane. The entire preparation was placed on the stage of the microscope and illuminated by transmitted light.

For use in stimulation, unipolar silver-glass micro-electrodes, one to five microns in diameter at the tip, were made by drawing a glass capillary containing silver wire (Fulton, 1941). The micro-electrode was placed in the field of the microscope by means of an Emerson micromanipulator. Brief faradic currents were passed through the electrode. The minute bubble of gas, which appeared at the tip of the electrode when the current was made and increased in size during the period of stimulation, served to indicate the moment and the duration of the stimulus. A water immersion lens was used, and, with a light-splitting prism, cinephotomicrographs were obtained at magnifications as great as 900 times.

Numerous preparations of the retrolingual membrane were stained differentially with methylene blue (National Aniline Company) for nerve fibers and perivascular cells, by adding three or four drops of a 0.5 per cent aqueous stock solution to approximately 10 c.cm. of Ringer's solution covering an excised preparation. The membrane was fixed in saturated ammonium picrate, mounted in glycerin and sealed.

Results

In the frog's retrolingual membrane, the vascular pattern consists of from two to four arterioles, which enter from each side and ultimately divide into anastomosing capillaries which converge laterally into venules. Examination of methylene blue preparations, stained differentially for perivascular cells, showed that the numerous typical smooth muscle cells of the arterioles became somewhat scattered on the precapillaries. The pericapillary cells, at the origin of certain capillaries, were branched and possessed nuclei arranged obliquely with reference to the capillary wall. Those found farther along the wall possessed longitudinally

arranged nuclei. Although the protoplasm of perivascular cells stained poorly, finely-branched cytoplasmic processes were seen frequently. A structural transition was evident from the typical smooth muscle cells on the arterioles, through the branched modified smooth muscle cells at the capillary origins, to the scattered pericapillary cells. In some capillaries there was a large extent of free endothelial surface. The perivascular cells on the venules had no definite arrangement.

The nature and distribution of the perivascular cells were also examined by means of stimulation with a micro-electrode. Most of the perivascular cells on arterioles, precapillaries and certain capillary origins contracted in response to direct stimulation and to indirect stimulation through a nerve. Except for an occasional pericapillary cell, those peripheral to the origin did not contract. The endothelium did not respond to mechanical stimulation, direct electrical stimulation, or indirect electrical stimulation through a nerve. Endothelial nuclei were not observed to swell and block the lumen of the capillary. The perivascular cells on small venules did not contract. Certain perivascular cells on the capillaries could be made to migrate a considerable distance along the wall by successive weak faradic stimuli. Others moved around the vessel, presenting a greater surface view of the nucleus, and creating the impression of nuclear thickening.

Examination of methylene blue preparations, stained differentially for nervous tissue, showed that non-myelinated nerve fibers, branches of the glossopharyngeal and hypoglossal nerve trunks, formed a loosely-meshed plexus, the primary or gross plexus, throughout the retrolingual membrane. Nerve fibers from the gross plexus approached the blood vessels and formed a perivascular plexus consisting of a long-meshed network, the secondary plexus, following the course of the vessels. This, in turn, formed a fine-meshed network, the tertiary plexus, on the vessel wall. The secondary plexus was abundant on the walls of arterioles and precapillaries, but sparse on the capillaries. Ganglion cells were not found in the retrolingual membrane. Many small fibrils from the tertiary plexus became progressively fine and

passed beyond the range of vision. Actual nerve endings were not found, although sufficient nervous tissue is present to innervate all the contractile elements. These observations confirm those of Woollard (1926) on the small vessels of the heart of the mammal.

Brief faradic stimulation of small non-myelinated nerves, composing the primary and secondary plexuses in the field of the microscope, gave evidence for smooth muscle motor-units, as well as for the presence of sphincter mechanisms at the capillary origins. Stimulation commonly produced spatially limited diphasic responses, dilation followed by constriction, in a specific vascular pattern. In control experiments, the micro-electrode was placed on the epithelium near a small nerve, but stimulation produced no response unless the electrode was put exactly on a nerve. Fulton and Lutz (1942) showed that, in the diphasic response, the constricted area was frequently only a portion of that originally dilated. Weak stimulation of a nerve sometimes produced only dilation, while strong stimulation of the same nerve might cause only constriction. Although the most frequent response to nerve stimulation was diphasic, a few vasomotor nerves produced either dilation or constriction to all strengths of stimulation. It is conceivable, therefore, that dilator and constrictor fibers are segregated occasionally near their terminations. These observations suggest the concept of discrete vasodilator and vasoconstrictor nerves with different thresholds of electrical stimulation.

In preparations treated with one per cent cocaine hydrochloride, stimulation of the vessel wall produced constriction in exactly the same vascular pattern as that produced by nerve stimulation before treatment. Cocaine made the nerve plexus non-functional, since stimulation of small vasomotor nerves produced no response. Consequently, the limited conducted response implies a non-nervous conducting mechanism, such as a muscle syncytium, discontinuous at the junctions of certain vessels. Our investigation shows that the musculature of the blood vessels of the frog's retrolingual membrane is composed of a large number of discrete smooth muscle motor-units. Extensive contractions of the blood vessels, when a nerve is stimulated, may

depend upon the activation of these motor-units coordinated through the nerve supply.

The contracting smooth muscle cells, following either direct or indirect stimulation, may be so completely coordinated that the responding vessels close simultaneously and abruptly. In other instances, portions of the vascular pattern, or of a single vessel, may be out of phase, resulting in one part being dilated, while an adjacent portion is being constricted.

Independent contraction of the capillary origin in a sphincter-like manner was commonly observed. Following nerve stimulation, pericapillary cells, at the origin of the capillary from the arteriole, or precapillary contracted to block capillary blood flow, either partly or completely. The capillary beyond this point always remained open. The contractile sphincter mechanism appears to consist of the modified smooth muscle cells which are stained at the capillary origins in methylene blue preparations. These cells are probably similar to, if not identical with, the contractile type of pericapillary cell reported by Rouget (1873), Vimtrup (1922), Tannenberg (1925) and others.

Spontaneous contractions of small blood vessels, confined to limited vascular patterns and to individual capillary origins, were frequently observed. Thus, smooth muscle motor-units may be rhythmic at times. Rhythmic contraction could be initiated by nerve stimulation, which, in certain instances, produced a contraction in exactly the same limited pattern, previously rhythmic. The cause of the spontaneous rhythmic activity of blood vessels, cut off from the central nervous system, was not determined. No ganglion cells were found in the methylene blue preparations. Furthermore, spontaneous rhythmicity occurred in lightly cocainized preparations, after stimulation of nerve fibers had ceased to give vasomotor responses. This suggests that spontaneous activity is a property of the smooth muscle of the blood vessels and of the modified smooth muscle cells of the capillary origin.

Summary

1. The nature and distribution of the contractile elements of

the small blood vessels in the retrolingual membrane of *Rana pipiens* have been examined by stimulation of vasomotor nerves and blood vessel walls with a micro-electrode, and by differential staining with methylene blue. In addition to the typical smooth muscle of the arterioles, there are modified smooth muscle cells at the capillary origins. Scattered perivascular cells beyond this point were generally inert. The endothelium did not respond to mechanical stimulation, direct electrical stimulation, or indirect stimulation through a nerve; nor did the endothelial nuclei swell to block the lumen.

2. A diphasic response, dilation followed by constriction, was generally produced in a limited vascular pattern by brief faradic stimulation of small vasomotor nerves, in the field under the microscope. The constricted region was frequently only a portion of that originally dilated. Some nerves gave only dilation, others, only constriction. These observations are interpreted on the basis of dual innervation.

3. Syncytial smooth muscle motor-units make up the musculature of the small blood vessels of the retrolingual membrane, since exactly the same limited vascular pattern responded to stimulation through the nerves and to direct stimulation of the vessel wall after cocainization. The perivascular nerve plexus is physiologically discontinuous.

4. Sphincter-like activity of the modified smooth muscle cells at the capillary origin occurred in response to brief faradic stimulation of small nerves, and spontaneously. The capillary always remained open beyond this point. Thus a mechanism is present, under nervous control, which may regulate capillary blood flow locally, even though the endothelium is inert.

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SECTION OF PSYCHOLOGY

FEBRUARY 16, 1942

DOCTOR RICHARD YOUTZ, Barnard College, Columbia University: *The Status or Certain Conditioning Principles after the Eclipse of 'Reflex Psychology.'* (This lecture was illustrated by lantern slides).

This evening, I should like to trace briefly the history of some principles initially discovered on the conditioned response. After that, we will consider a few experimental situations where Pavlovian conditioning itself is unimportant, but where these principles seem to apply.

Interest in the conditioned response began in this country in connection with a kind of crusade to bring objectivism to psychology. It was used as a new and objective technique, but it was also used as an explanatory device in places where, as we now suspect, it did not belong. Although the Pavlovian conditioned response is now little used as a building block in psychological theory, the *characteristics* of the conditioned response have attained a life of their own in application to a large number of different problems. When I speak of "characteristics," I refer to such things as extinction, spontaneous recovery, secondary extinction, and generalization. For instance, these characteristics have been found in learning by reward. They are also useful in describing trial-and-error learning, and the Gestalt transposition experiment. Some of them appear in rewarded verbal learning, and there are indications of their presence in certain simple kinds of concept-formation and, perhaps, in problem solving. I shall not attempt to review the literature on these topics, but will choose typical and, I hope, cogent examples. The literature has been amply reviewed by a number of writers, among them, Razran, Hilgard and Marquis, and Skinner.

The heyday of the conditioned response, as an all-explanatory

principle in psychology, was during the 1920's. Watson had used it in his presidential address as a substitute for introspection and presented it as an important technique in the study of emotion, mental disease, the formation of language, and habit structure. During the 20's, the conditioned response spread into many fields as an explanatory principle, before experimental findings offered much sound support. In 1921, Smith and Guthrie published a general textbook which used the conditioned response in place of a more general associative theory. In 1924, F. H. Allport described many of the phenomena of Social Psychology in terms of the conditioned response. In the same year, Burnham's book, "The Normal Mind," presented it as a useful explanatory principle in the field of mental hygiene. These and many others seized upon the new concept and applied it to various theoretical problems.

However, experimental verification did not keep pace with theoretical description. It was found, for instance, that maze learning was not easily fractionated into a series of chained reflexes. In human beings, the desire for money was certainly not inherited, nor was it apparently a second-order conditioned response. The second-order responses set up in the laboratory were too tenuous and transitory to be examples of strong derived drives. Finally, reward and punishment learning (or law-of-effect learning), did not fit the conditioned response pattern and many data were best described in terms of reward and punishment. The Pavlovian picture was found to be inadequate and the first wave of enthusiasm passed.

The law of effect had long been important in trial-and-error situations and had been well-known since E. L. Thorndike's experiments and theoretical formulations, beginning in 1898. Now it was found that Thorndike's Law of Effect and Pavlov's conditioning principles dealt with very similar situations. As they were compared, it became apparent that, while they were concerned with similar problems, they differed as to the particular stimulus-response connection that was strengthened during learning. In the conditioned-response paradigm with S_1, the bell,

and S_2, the meat-powder, the animal learns to connect S_1 with R_2. R_1 is ignored. In the Thorndikian paradigm, the connection between S_1 and R_1 is strengthened when S_2 and R_2 are reward. The connection between S_1 and R_2 is largely ignored. The question now arose of whether the S_1-R_2 connection of Pavlov and the S_1-R_1 connection of Thorndike had characteristics in common and acted in the same way in a variety of experimental conditions. Specifically, did Thorndike's rewarded learning exhibit extinction, recovery, secondary extinction, and so on? Since we will deal with these concepts a number of times, I'll describe them briefly. In conditioning, S_1 comes to evoke R_2 through repeated presentations of S_1 and S_2. In extinction, the conditioned responses die out when the S_1, or bell, is presented without S_2, the meat powder. After the weakening of a conditioned response in extinction, it will recover some of its strength, if the experimenter simply waits, and presents no experimental stimuli to the subject. This peculiar property is known as "spontaneous recovery." In "secondary extinction," the extinction of one conditioned response in a particular experimental subject also weakens other conditioned responses in the same subject. If a parallelism between conditioning and reward learning is established, experimental findings on either of these kinds of learning have a much wider area of possible application.

As a result of a large number of investigations, it has been found that rewarded learning *does* exhibit many of the characteristics of the conditioned response. Grindley found that learning occurs if a guinea-pig is rewarded with a piece of carrot when it turns its head to the right at the sound of a buzzer. (This is very similar to Thorndike's experimental findings, with the difference that, in Grindley's procedure, the head-turning movement occurred after a specific buzzer-stimulus.) Grindley also found that this head-turning tendency is subject to extinction, if the carrot-reward is not given. Skinner has made extensive investigations of rewarded behavior. In his experiments, a rat is placed in an experimental box and trained to push down a small lever. Depression of the lever releases a piece of food which constitutes

a reward for the rat. The animal soon learns to continue pressing the lever to obtain food. When the food-release mechanism is disconnected, the rat continues to press the lever and the records show a typical extinction curve, with the rat finally making only occasional responses. Skinner has also found that, if the rat is removed from the experimental box following extinction and then replaced after an interval, it again presses the lever repeatedly, showing spontaneous recovery. With this experimental technique, Skinner has studied discrimination-learning and a number of other related phenomena. Using a Skinner-type experimental box, the speaker has found two further Pavlovian principles in rewarded learning, namely, secondary extinction and a rise in the retention curve. In the secondary-extinction experiment, either of two levers can be placed in the box, one horizontal and one vertical. It was found that after the rat had learned to operate each of these levers separately, extinction on one of them would reliably decrease the number of extinction-responses on the other.

The rise in the retention-curve for rewarded learning parallels a similar rise for the conditioned response, which seems not to follow the familiar retention curve for learned verbal material.

Almost all of the above findings have been confirmed by independent experimenters. Because of this functional similarity between rewarded learning and conditioning, the terminology has also changed. The majority of writers on the subject consider them both as forms of conditioning. Miller and Konorski suggest that rewarded learning be regarded as a second type of conditioned response. Skinner considers rewarded learning as more important and describes it as a Type I conditioned response, with the Pavlovian sort as Type II. Hull has derived both sorts of learning from more general associative principles and regards conditioning as a special and limiting case of learning by reward. Schlosberg, on the other hand, distinguishes carefully between the two kinds of learning, while pointing out the similarity of their results. Razran considers rewarded learning as qualitative, and directional conditioning, with conditioning itself, as quantitative and basic. Hilgard and Marquis describe rewarded learning as

"instrumental conditioning" in contrast to the classical Pavlovian sort. There seems to be no doubt that rewarded learning is changing its name.

And now, what have been the advantages of the extension of these conditioning principles into a new field? We may use two situations as representative.

In Trial-and-error Learning

The first is in trial-and-error learning. In Thorndike's formulation, the animal is placed in a problem-situation, in which it can make a number of different responses. For instance, there is the cat in the problem-box, in which it has to push a button to open the door to food. Each trial is terminated when the cat makes the correct response and gets food. On successive trials, the cat takes less and less time to arrive at the correct response and finally eliminates the incorrect responses and does the correct one as soon as it is put in the problem-box. This is certainly one of the commonest forms of learning in animals and humans.

Hull has applied conditioning principles to trial-and-error learning and has suggested several questions to be answered by any explanation of it.

1. Why does the organism frequently repeat unrewarded responses? *i.e.*, instead of trying some other response?

2. When a response has been rewarded, why is it not immediately fixed upon and continued without interruption? If an organism is successful on the last response of one trial, why does it make several wrong responses at the beginning of the next trial?

3. What is the order of eliminations of the unrewarded responses?

We can give a qualitative description of trial-and-error learning and answer these questions if we apply principles of reinforcement, extinction, and recovery.

Let's assume that the cat is in the problem-box and three of its responses are: *1.* Jumping up and bumping its head against the top of the box; *2.* Miaowing pitifully; and *3.* Scratching at the release-button. There are, of course, many other responses

and gradations of these, but we'll take only a few for illustration.

Let us further assume that this particular cat has most often been successful in escaping from boxes by *bumping its head* against the top. Because of this frequent success, its strongest reaction-tendency is toward bumping its head. It has been successful less frequently when using the method of *miaowing pitifully*, so that this reaction-tendency is not quite so strong. And, finally, it has only occasionally escaped by *scratching* at the side of the box near the door (and the release button), so that this reaction-tendency is the weakest of the three.

When the cat is put into the problem-box, it first bumps its head against the top, because this has been most often rewarded in its previous experience. It bumps its head more than once (Question 1) and, in fact, continues to do so, until the head bumping tendency is weakened by *extinction* to a strength less than that of the miaowing tendency. It then miaows several times. But, while it is miaowing, the head-bumping tendency undergoes *spontaneous recovery*. The cat again bumps its head. The head-bumping and miaowing continue until both tendencies are reduced below the level of strength of the scratching tendency. It then scratches at the door, is released, and obtains a bit of fish which constitutes a reward.

When the cat is put into the problem-box for the next trial, it would, if it were entirely rational, immediately scratch the release button. However, the head-bumping and miaowing tendencies have recovered part of their original strength and the cat again performs those acts. (Question 2.) It reaches the release button more quickly this time, because the head-bumping and miaowing are more quickly extinguished below the level of the successful response. This sequence is repeated for successive trials, until the cat scratches the release button immediately after being placed in the box, and the problem is then considered solved. To answer Question 3, we must know the original strengths of the tendencies and the shape of the curves for successive extinctions and rewards for the particular animal, in this particular situation.

Secondary extinction probably also operates here, reducing

the strength of all of the response-tendencies when one of them is being extinguished.

This is a most irrational picture and one's tendency is to say —"This happens in animals, but humans don't follow this pattern. Human beings act rationally." This is extremely doubtful. Let me illustrate with the picture of a man hunting for theater tickets just before he leaves home for the theater. Usually, in the past, the man has put the tickets in his top bureau drawer. Occasionally, he has left them in his vest-pocket; but this time, with special care, he put them in his overcoat pocket.

When he is about to leave for the theater, our friend goes confidently to the bureau drawer. The tickets aren't there. He looks again, more carefully, and still more carefully. Then he goes through his vest pockets. In the meantime, the bureau-drawer tendency is recovering. He looks in the drawer again. After going back and forth several times, he "remembers" that the tickets are in his overcoat pocket and finds them. His behavior is very like that of the cat in the problem-box on a single trial.

The action of the separate variables is difficult to distinguish in the records of an actual trial-and-error situation. However, I have some curves which, at least, show the effects of recovery and extinction. Ten rats were trained to push down a horizontal lever or bar, to get food, and also trained to push a vertical bar sideways to obtain food. Each rat was given 60 rewarded trials on each bar alone. After this training, the rat was put in the experimental box with *both* bars present. The response to one bar, the vertical for instance, is rewarded, while that to the horizontal bar is not. The rat is allowed a *total* of 30 responses a day on both bars together. This is repeated, day after day, until, on one day, 28 out of the 30 responses are made on the rewarded bar—the vertical, in this instance. After this criterion is reached, the situation is reversed, and responses to the horizontal bar are rewarded and those to the vertical not rewarded. Ten rats were run on this schedule for 74 consecutive days. These rats took varying numbers of days to reach the criterion of 28 rewarded out of 30 responses. If all those cases in which a rat took 4 days to

reach the criterion are pooled, some interesting regularities appear.

The 30 responses on each day were divided into six groups of five each. Then the average number of non-rewarded responses in each successive group of five was determined. The decrease in number of non-rewarded responses on any one day is only partly the result of extinction, since the other responses in each group of five were rewarded. Thus, the drop in each curve is the result of strengthening of the rewarded activity and also of the extinction of the non-rewarded activity. However, the rise in non-rewarded responses, from the end of one day's record to the beginning of the next, seems to be the result of recovery. This is the case since you recall that the retention curve for this kind of learning does not fall, but rises. These curves represent the complex resultant of both reward and non-reward (and undoubtedly other factors as well). If we take the information gained in studying the learning, extinction, recovery, and retention of responses to one bar alone and combine them for the two-bar situation, we should expect this sort of extinction and recovery, from one day to the next. And apparently it does occur.

The Transposition Experiment

There are other applications of conditioning principles to complex forms of rewarded behavior. One of these is in discrimination learning, especially concerning *relative* as contrasted with *absolute* responses to stimuli. The "transposition experiment" provides an example. An animal is presented with two stimulus cards (A and B), of different size, and trained to respond to B, the larger. Now, if it is presented with card B and a still larger card, C, the animal will make a majority of its responses to card C, even though it has never been rewarded for responding to C. Response to card C is a relative discrimination here, and response to card B would be an absolute one. This experimental finding has been used in Gestalt arguments against association theories, on the ground that, in relative responses, the animal is responding to a *relational* stimulus, an aspect of the total situation. The problem is complicated by the fact that, if card B is presented to the

animal in conjunction with a much larger card, E, the animal will choose card B, an absolute response.

In a theoretical approach to the problem, Spence found that both relative and absolute choices could be accounted for, if the conditioning principles of positive and negative generalization were used. Later experimental findings have modified the shape of the generalization curves, but the relationships are still as originally presented by Spence.

The figures on the base-line in Spence's curves represent areas of stimulus cards in sq. mm. The two stimulus-cards, A and B, of the above example, have areas, we may say, of 160 and 256 sq. mm. respectively. In the initial training, the animal is rewarded when he responds to card 256, and not rewarded when he responds to card 160. When the animal is learning to respond to card 256, it is also acquiring a tendency to make the same positive response to other similar stimuli. The tendency diminishes as the stimuli differ in the size from card 256. This is the positive generalization curve. On the other hand, the animal is *not* rewarded when it responds to card 160. While it is learning not to respond to card 160, it is also acquiring a tendency *not* to respond to other similar stimuli as represented on the baseline. This tendency *not* to respond diminishes as the stimuli differ in size from card 160. This is the curve of negative generalization, or, as it is sometimes called, the generalization of inhibition. Now, let us suppose that the animal has been trained on this size discrimination, *i.e.*, he has a strong tendency to respond to card 256 and another, weaker tendency, not to respond to card 160. In order to predict his activity when we present him with a different pair of cards, we must consider the algebraic sum of his tendencies *to* respond and *not* to respond at each stimulus value. Using this theoretical structure, it is possible to predict the pairs of stimuli to which the animal will make an *absolute* response and those stimuli to which it will make a *relative* response. In addition to this, it was found that, with some pairs, the animal would make neither an absolute nor a relative response.

While this is a nice theoretical picture, the question is: Does

it work out experimentally? The answer is: With certain qualifications, yes. With adjustment of the shape of the generalization curves, Spence has successfully applied this theory to discrimination learning in chimpanzees. Jackson and his students have performed a series of experiments on discrimination in children, aged 2½ to 6, in which these same principles were found to apply. It was further reported that the shape of the generalization curves is influenced by the amount of preliminary training, the spread of the curves being less, the greater the amount of training.

Trial-and-error learning and discrimination-learning are two of the better verified extensions of conditioning principles to more complex forms of behavior. I should now like to present some evidence that extinction and recovery also occur in rewarded verbal learning in humans.

Rewarded Verbal Learning

It has been found by Thorndike, Lorge, Rock, and others, that, when a subject is presented with a list of words and asked to give a number between one and eight for each of the words, the subject learns those word-number connections which are called "right" by the experimenter.

However, in the same situation, if a number-response is called "wrong" by the experimenter, the subject does *not avoid* it, and even makes the same number-response, on later trials, with better than chance frequency. This startling finding has been repeatedly confirmed.

It will be noted that the number-responses, which were not avoided upon being called "wrong," were probably not connected with the particular word by any strong habit structure. The subject would have had little opportunity or need to associate any particular number between one and eight with the words in the experiment.

This raises the question of what would happen to a response that was already learned, if it were repeatedly called "wrong" by the experimenter.

Twenty-one college students served as subjects. Reading

from a forty-word list, the experimenter said a word, and the subject responded with any number between one and eight. The experimenter then said "right" or "wrong" according to the number's predetermined correctness. This list of forty words was repeated eight times in this manner. Altogether, each subject was given eight trials on each of three different lists. Each subject was then dismissed and told to return two days later at the same time.

The experimenter then noted all the word-number connections which conformed to the following criteria:

1st—The connection had been correctly made 3, 4, 5, or 6 times during the eight trials, with the added provision that only those connections were chosen which had occurred correctly in the last three of the eight trials.

2nd—Connections occurring in the first four words or in the last four words of a list were not used.

3rd—No connections were used which had been made correctly on the first trial. This was done in order to rule out, as far as possible, associations already established in the subject by previous experience.

In the records for the 21 subjects, there were 147 word-number connections which fulfilled these requirements. *These 147 connections were divided into two groups* with approximately equal numbers of the 3, 4, 5, and 6-time correct responses, in each group. Seventy-one connections were assigned to the "Reward" group and 76 to the "Punishment" group.

Two days after learning, a second series of eight trials was given. The procedure was substantially as before and the subjects were not told that anything would be changed. The "reward" group was treated exactly as before. However, the responses of the "punishment" group were called "wrong." All other number-responses were treated as before.

Twenty-four hours after this second series, another series of five trials was run, in which the procedure was exactly the same as in the second. This third series was run in order to test for recovery between the last trial of the second series and the first

trial of the third. This was on the punished connections, of course.

The changes that interest us are those that occurred during the second and third series. On the first trial of the second series, the subjects show that they have retained about 58% of the connections learned two days before. Those that are rewarded by "right" are retained and the subjects then proceed to relearn many of those that had been forgotten. *After* the second series, some are again forgotten and partially relearned in the third series. The connections that are "punished" drop from 58% to 16%, showing a sharp drop in the early punishment, or extinction, trials. The drop in this case is 42 per cent points.

During the 24 hours between the second and third series, the punished responses rise in correctness by 14 per cent units. This rise has over 98 chances in 100 of being a true one and is shown by all of the various groups of connections that were correct 3, 4, 5, and 6 times during the eight learning trials. Moreover, on the 8th trial, the punished responses have dropped only to 16%, so that the subsequent rise cannot be attributed to a return to a chance level of responding. Chance in this case is 12½%. *Apparently, recovery is a characteristic of this sort of learned-and-then-punished connections.*

It is also interesting to examine the relative resistance to extinction of the connections rewarded 5 and 6 times and those rewarded 3 and 4 times. Although the 5 and 6 reward curve is not much higher than the 3 and 4 curve, the direction of the difference is maintained in 10 of the 13 possible instances. The amount of original training seems to affect the resistance to extinction, a finding similar to that found in studies of the lever-pressing response in rats. A greater irregularity of the 3 and 4 curve may be the result of the smaller N, or it may be that with a smaller number of original rewards, the connections do not respond to punishment in the same way. The latter possibility seems more likely, since Thorndike and others have found that with *no* original learning, verbal punishment has no decremental effect.

Calling a word-number connection "wrong," and lack of

reward when a rat presses a bar, are certainly not the same, but they may be analogous. When a rat has been receiving food following depression of a lever, and then, suddenly, no food appears after a response, there is certainly an objective lack of reward. We do not know what the rat "thinks" about it, of course. In word-number studies on college students, however, if the number response is followed by neither "right" nor "wrong" but only by an uninformative word such as "next," the situation is different. Questioning the subjects reveals that they are just as likely to believe they were right, although a number of them say they do not know what it means. The word "next," following a number-response, certainly gives no objective indication of success or failure.

In order to determine the effect, if any, of the word "next" on previously rewarded word-number connections, an experiment was run with the aid of Miss Jean Ackermann of Barnard. The procedure was the same as before, except that, on the second day, the word "next" was substituted after 19 word-number connections. Only those connections were used, which had been correct 5 or 6 times during the first day's training. When "next" is used, no great change occurs in the number of correct responses. There is a slight drop in the retention curve during a 24 hour retention-interval, but not greater than would be expected on verbal material that was learned 48 hours before. In comparing these results with those for lever-pressing in rats, it is probably true to say that there is nothing in the rat situation which is analogous to "next" in the word-number experiments. Possibly, however, punishment in the verbal situation is analogous to extinction for the rats.

One of the other principal points of difference between the rat lever-pressing and the word-number connection, is that, with word-number connections, the retention curve falls, as in retention of other verbal material. If we assume the interference theory of forgetting, the difference seems a little clearer. After college students have learned word-number connections, they continue to use words and numbers during the retention interval. There is interference, or "retroactive inhibition," and "forgetting"

occurs. On the other hand, the rats live in their cages during the retention interval and have little opportunity to manipulate levers. This might account for the lack of drop, but does not explain the rise in the rat's retention-curve, which has to be interpreted in the light of other principles.

Other Applications

In this discussion, I have not as yet mentioned the greatest single application of conditioning principles to the field of verbal learning. This is the "Mathematico-Deductive Theory of Rote Learning" by Hull, Hovland, Ross, Hall, Perkins, and Fitch. It did not seem possible to compress it into ten minutes, or even fifteen minutes. This work began with an assumption which is easily stated. It is as follows: *In rote learning, the remote associative bonds of Ebbinghaus have the characteristics of the Pavlovian trace conditioned response.* With this as a starting point, and using a large number of other principles as well, Hull has presented a series of progressive approximations toward an adequate statement of the principles of rote learning. The "Mathematico-Deductive Theory" is the latest approximation, more adequate because thoroughly quantitative, but still, as he points out, an approximation. It is one of the most complete, if not *the* most complete, statements of assumptions, inferences, and experimental relationships in any field of psychology today.

In the field of concept-formation, there are at present only suggestions. Humphreys has introduced a kind of conditioning experiment which he calls "Expectancy learning." It is also describable as a very simple form of concept formation and it also shows extinction and spontaneous recovery.

In problem-solving, we have heard since Ruger's experiments and before, that problem-solving is "mental trial and error." It is possible that the various "hypotheses" that occur to the problem-solver may appear in an *order* which is like that found in trial-and-error learning in rats or in theater-ticket behavior in men. In a recent conversation, Keller reported that human subjects, responding in a modified Skinner-box situation, show a

change in hypothesis as a change in the slope of the curve of cumulative responses. Interestingly, the slope sometimes changes *a little before* the subject can verbalize the new hypothesis. There are other experiments now in progress which are attempting to control the appearance of hypotheses in problem-solving by subjecting them to strengthening and extinction procedures.

In this report, the goal has been to show some of the representative areas in which conditioning principles have been found useful. The conditioned reflex itself is now seen to be only a very minor aspect of learning, if indeed it is a kind of learning. The principles, which have spread so widely, are themselves frequently questioned. Extinction may soon be considered as a special case of learning, learning of an alternative incompatible response. Spontaneous recovery would then be merely another retention curve, and secondary extinction would be a kind of positive generalization or transfer. Generalization, itself, occurs along stimulus-dimensions which may possibly be more adequately approached by psychophysics. However, in terms of present formulations, conditioning principles have brought light and explanatory power to a fairly wide range of psychological problems.

SECTION OF ANTHROPOLOGY

FEBRUARY 23, 1942

DOCTOR CORA DU BOIS, Sarah Lawrence College, Bronxville, N. Y. and DOCTOR EMIL OBERHOLZER, New York, N. Y.: *Rorschach Tests and Native Personality in Alor, Dutch East Indies.*

This study is an attempt to apply Rorschach's experiment cross-culturally. The conclusions are based on the fact that the Alorese Rorschach findings are completely different from those of normal Europeans and Americans. The study was confronted with this difficulty from the beginning. The approach to some of the fundamental features in Alorese psychology was possible only because of experience with Rorschach tests in psychopathology.

The test results of the Alorese differ in almost every experimental factor from those of Americans and Europeans. In addition, the Alorese exhibit specific reactions which are missing in normal Europeans and Americans. Apart from the Alorese, these reactions occur only in certain pathological states of mind. However, they never occur to the same extent as among the Alorese, where they are part and parcel of their reactions to the non-colored cards of Rorschach's ten ink blots. They come nearest to the findings in the defective states of the traumatic constitution. This offers the clue to an understanding of the specific reactions in the findings of the Alorese.[1]

The Alorese Rorschachs show no movement answers. These answers indicate that the picture or parts of it are seen in movement. Movement answers, in conjunction with color responses, show the stability of a person's emotions to the point of complete

[1] Oberholzer, E. Zur Differentialdiagnose psychotischer Folgezustände nach Schädeltraumen mittels des Rorschachschen Formdeuterversuchs. Zeitschrift f. d. ges. Neur. und Psychiatrie, 1931. See also "Report on Rorschach's Experiment in Traumatic Mental Disorders" at the first International Neurological Congress in Berne, 1931.

self-control and balance of mind. In addition, the proportion of the various color reactions are completely reversed in Alorese and European tests. The form colors represent emotional responsiveness and adjustment. The color forms and the primary colors (such as blood, water, etc.) are the expression of egocentric affectivity and impulsiveness which suffer no control and adaptation. The form color, which prevails in findings of normal Europeans and Americans and which constitutes nearly one half of all the color reactions in European material, is missing in the Alorese. In its place, occur the color form and, still more, the color responses. These do not occur in normal Europeans whereas they amount to almost one half of the color reactions of the Alorese. That is, their color form and color about equal the amount of European form color. The singularity of the Alorese findings, therefore, is best evidenced by the fact that they would represent pathological conditions in Europeans and Americans.

The conclusions to be inferred are that the personality and emotional character of the Alorese are very different from normal Europeans and Americans. The Alorese are fearful and distrustful. Measured by our responsiveness, they lack inter-individual contact and rapport, and possess an excess of emotional reactions which occurs only in American and European psychotics or serious neurotics. Last, but not least, the Alorese have an attitude of passive acceptance. They differ from Europeans and Americans in respect to the degree and kind of intelligence. As to its degree, they rank on the level of our morons, or, at the best, of our most unintelligent. The intelligence of most of the Alorese is equivalent to that of the feeble minded among Americans and Europeans. Nevertheless they are neither morons nor feeble minded, just as they are not schizophrenics or traumatics. Their Rorschachs and psychology are something *sui generis*. Among themselves, in their culture and milieu, they are adjusted and their Rorschachs represent their norm.

From the tests, it is evident that, within the range of the characteristic personality structure, there are wide individual differences. The individual personality of the Alorese can be

inferred from the varying number of color reactions just as it can be for an American or European. The latitude of their variations is even considerably greater than in Europeans and Americans.

There is only one quantity among the numerical values of the experimental factors that the Alorese have in common with Europeans. The form answers, i.e., those interpretations which are determined by the form and outlines of the pictures or their parts, are not only the majority of all the answers, but occur in the same percentage among the Alorese and Europeans. It remains to be seen whether other natives, as different from the Alorese as the Alorese are different from the Europeans, exhibit the same striking fact.

SECTION OF PHYSICS AND CHEMISTRY

FEBRUARY 27 AND 28, 1942

Conference on "*Physics of the Solid State.*"

The Section of Physics and Chemistry held a Conference on "Physics of the Solid State," as the third in the series for the Academic Year of 1941–1942. Professor P. Debye, Cornell University, Ithaca, N. Y., was the Conference Chairman in charge of the meeting.

The program consisted of the following papers:

"Introductory Remarks," by P. Debye.

"Co-operative Phenomena in Solids," by J. G. Kirkwood, Cornell University.

"Thermal Disorder in Crystal Lattices," by H. W. Zachariasen, University of Chicago.

Structure and Degree of Order in Glass," by B. E. Warren, Massachusetts Institute of Technology.

"Structure and Order in Macromolecular Solids," by W. O. Baker and C. S. Fuller, Bell Telephone Laboratories.

"The Plasticity of Crystals and Lattice Imperfections," by Frederick Seitz, University of Pennsylvania.

"Paramagnetic Relaxation," by J. H. Van Vleck, Harvard University.

"Recent Progress in the Study of Co-operative Phenomena," by G. H. Wannier, University of Iowa.

"Molecular Rotation in Some Mixtures of Crystalline Organic Solids," by S. O. Morgan, Bell Telephone Laboratories.

"Absorption of Elastic Waves in Solids," by H. S. Sack, Cornell University.

ANNOUNCEMENT OF PUBLICATIONS FOR 1942[1]

The following publications will be issued by the Academy during the current year. Members of the Academy who desire to receive these papers will kindly request the Executive Secretary to send them, and they will be mailed, free of charge, as they are ready for distribution, except as qualified in the footnotes:[2]

ANNALS:

1. "Thermodynamic Properties of Solutions of Long-Chain Compounds," by Maurice L. Huggins. (Approximately 35 pages.)
2. "Immunochemistry." Papers delivered at the conference by this title. (Approximately 100 pages.)
3. "Studies on Fresh Water Bryozoa, XII. A Collection from Various Sources," by Mary D. Rogick and Claudeous J. D. Brown. (Approximately 30–40 pages.)
4. "An Analysis of the Locomotion of the Sea Horse (*Hippocampus*) by Means of High Speed Cinematography," by Charles M. Breder, Jr. and H. E. Edgerton. (Approximately 50 pages.)
5. "The Ultracentrifuge." Papers delivered at the conference by this title. (Approximately 100 pages.)
6. "Protein Hormones of the Pituitary Body." Papers delivered at the conference by this title. (Approximately 100 pages.)

TRANSACTIONS:

Series II, Volume 4, Nos. 1–8. These papers are sent to all members of the Academy, regularly, during the academic year. (Approximately 200–250 pages.)

[1] Notice of additional publications to be added to this list will be sent to members later.

[2] Active, Sustaining, Life, and Honorary Members may receive, upon request, a copy of all current numbers of the Annals.

Student and Associate Members are entitled to receive one complete monograph or up to 150 pages of smaller papers.

Scientific Survey of Porto Rico and the Virgin Islands:[3]

Volume XIX, Parts 1 and 2: "Meteorology of Porto Rico," by Robert G. Stone.

Volume XIX, Part 3: "Meteorology of the Virgin Islands," by Robert G. Stone.

[3] The Scientific Survey publications are available to Members for $1.50 per part and to non-members for $2.00 per part.

A certain number of parts, as announced each year, are available free to Honorary, Life and Sustaining Members.

NEW MEMBERS

Elected February 26, 1942

LIFE MEMBER

Dubos, Rene J., Ph.D., Sc.D. (hon.), Bacteriology, Biochemistry, Member, Rockefeller Institute for Medical Research, New York, N. Y.

ACTIVE MEMBERS

Ballintine, Robert, Ph.D., Physiology, National Research Fellow, Rockefeller Institute, New York, N. Y.

Barker, George E., Ph.D., Organic Chemistry, Industrial Fellow, Mellon Institute, Pittsburgh, Pa.

Barnes, R. Bowling, Ph.D., Physics, Director, Physics Division, Stamford Research Laboratories, American Cyanamid Company, Stamford, Conn.

Barnes, Robert Percy, Ph.D., Organic Chemistry, Associate Professor, Organic Chemistry, Howard University, Washington, D. C.

Benaglia, Angelo E., A.B., Biochemistry, Research Toxicologist, American Cyanamid Company, Stamford, Conn.

Byrns, Ruth, Ph.D., Psychology, Director of Teacher Training, Fordham University School of Education, New York, N. Y.

Count, Earl W., Ph.D., Anthropology, Professor, Anatomy, New York Medical College, New York, N. Y.

Dietz, Thomas J., M.S., Physics, Assistant Director, Biochemical Research Foundation, Newark, Del.

Doll, Alfred Wm., Ph.D., Engineering, Head, Department of Physics, Pratt Institute, Brooklyn, N. Y.

Doyle, Winfield G., M.A., Zoology, Physiology, Supervisor of Radio and Sound, American Museum of Natural History, New York, N. Y.

Emerson, Kendall, Jr., M.D., Biochemistry, Medicine, Assistant Resident Physician, Rockefeller Hospital, New York, N. Y.

Emley, Warren Edwards, Ch.E., Chemical Engineering, Chief, Division of Organic and Fibrous Materials, National Bureau of Standards, Washington, D. C.

Eyster, William H., Ph.D., Genetics, Professor, Bucknell University, Lewisburg, Pa.

Fox, Jay T., General Zoology, Secretary to Supreme Court Justice Schmuck, New York, N. Y.

Friedheim, Ernest A. H., M.D., Ph.D., Chemotherapy, Pathology, Research Assistant in Chemistry, Mount Sinai Hospital, New York, N. Y.

Goldstein, Louis, Sc.D., Physics, Instructor, Physics Department, College of the City of New York, New York, N. Y.

Gregory, Douglas W., Reg.Ind.Eng., Physics, Chemistry, Biology, Psychology, Head, Research Laboratory, Firestone Tire and Rubber Company, Wyandotte, Mich.

Harding, Floyd E., M.D., Endocrinology, Ross-Loos Medical Group, Los Angeles, Calif.

Lombardo, Carmyn J., M.D., Medicine, Lecturer, Fordham University Graduate School, New York, N. Y.

Mount, Adela Pond, M.A., Geology, Retired, Upper Montclair, N. J.

Chapman, George H., Bacteriology, Director, Clinical Research Laboratory, New York, N. Y.

Orth, John C., Assistant Curator, Department of Education, American Museum of Natural History, New York, N. Y.

Robinsion, Ellis J., Ph.D., Physiology, Research Physiologist, American Cyanamid Company, Stamford, Conn.

Weber, Ernst, Ph.D., D.Sc., Electrical Engineering, Physics, Professor, Graduate Electrical Engineering, Head, Graduate Study and Research in Electrical Engineering, Polytechnic Institute of Brooklyn, Brooklyn, N. Y.

Weber, Ione, Ph.D., Biochemistry, Assistant Professor, Brooklyn College, Brooklyn, N. Y.

Zinn, Donald J., M.S., Ecology, Oceanography, Graduate Student, Yale University, New Haven, Conn.

ASSOCIATE MEMBERS

Bradley, William F., Ph.D., Geology, Chemistry, Physics, Associate Chemist, Illinois State Geological Survey, Urbana, Illinois.

Cagle, Fred R., M.S., Herpetology, Assistant Professor, Southern Illinois Normal University, on leave as University Fellow, University of Michigan, Ann Arbor, Mich.

Conover, Courtney, B.S., Chemistry, Research Chemist, Mansanto Chemical Company, St. Louis, Mo.

Coxe, Warren W., Ph.D., Psychology, Director, Division of Research, State Education Department, Albany, N. Y.

Cummins, Arthur B., Ph.D., Chemistry, Mineralogy, Section Chief, Johns-Manville Research, Johns-Manville Corporation, Manville, N. J.

Day, Harry G., Sc.D., Biochemistry, Associate Biochemist, School of Hygiene and Public Health, Johns Hopkins University, Baltimore, Md.

Dott, Robert Henry, M.A., Geology, Director, Oklahoma Geological Survey, Norman, Okla.

Evers, C., M.S., Biophysics, Professor, Biology, Physics, Central College, Pella, Ia.

Grobman, Arnold Brams, M.A., Zoology, Teaching Assistant, Biological Laboratories, University of Rochester, Rochester, N. Y.

Hoard, James Lynn, Ph.D., Physical Chemistry, Assistant Professor of Chemistry, Cornell University, Ithaca, N. Y.

Jacobson, Werner, Chemistry, Research, Denver, Colo.

Love, John David, Ph.D., Geology, Shell Oil Co., Centralia, Ill.

Mosley, Vernon M., A.B., Physics, Washington, D. C.

STUDENT MEMBER

Ward, Warren Thomas, B.S., Histology, Laboratory Technician, U. S. Navy, New York, N. Y.